

April 2, 1950.

Dr. B. D. Davis,
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Dear Bernie:

I just saw your note in MGB on your further tests for a soluble transforming agent in K-12. I am glad that someone has gotten round to do this. In my thesis, but not-I discover- in the 1947 Genetics paper, an experiment is mentioned which reached the same conclusion: merely that the parents were plated one over the other in thin agar films rather than mixed. The yield was reduced so drastically that the motility of the reacting components must be quite low, and comparable to that of the bacteria themselves. Your experiment should be perfectly feasible even with a filter that leaks a few bacteria, and might even be more convincing: it would then be possible to flush the supernatant back and forth rather frequently [which, by the way, I thought was the most ingenious part of your experiment].

This letter is to ask you whether you are contemplating the publication of your note. I would like very much to encourage you to submit its substance as a letter or note to J. Bact. There are still some die-hards to whom this experiment, plus the confirmation of the general results in another laboratory, would be convincing. Aside from this consideration, which you can take to be a more general benefit, the experiment itself is a small piece of ingenuity for which you should be credited. Papers of a confirmatory character are sometimes difficult to have accepted for publication, but I am sure that there would be no difficulty here.

If you do publish, I would appreciate it, as a personal favor, that you recast the discussion so as not to bring in the speculation [which I don't take too seriously, and which is liable to be confused with the heretical idea that the phage is the gamete] of a smaller gamete on the basis of the transmission of lambda. These experiments are still very preliminary, and I think it would be better not to refer to a speculation for which there is no evidence until it finds [or fails to] more definitive support. The gradation between TP and gamete is taken up in some detail in my 1947 paper, and I think your experiment has some importance in setting some sort of bound to the size [or the lability] of the reactants, which are discussed there only in much more general terms.

If you can spare them, I would greatly appreciate a couple of duplicate reprints of your *Experientia* paper: this will save a lot of headaches in assigning reading materials for class purposes, and I think this is one broad paper that the students should read.

Sincerely